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ABSTRACT

Presented are seven child research and development papers delivered at the 1973 American Psychological Association Symposium in Montreal. Described are the beneficial results produced by relevant goal oriented researchers who become directly involved with societal problems. Advocated is the need for psychology to become more historical, empirical, and pragmatic within an ethological framework. Research and development centers such as the one at the University of Minnesota are set forth as alternative arrangement to traditional departments. Discussed is the relationship between an early intervention project for retarded children and a university research and development center. The connection between theoretical and applied research is examined. Emphasized is the importance of adjusting research to fit practical problems in such real-life situations as the home and the classroom. (CL)

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1. D. Moores, M. Goodwin & K. Weiss. Evaluation of programs for hearing impaired children: Report of 1972-73. Research Report #57. December 1973.

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3. N. Buium. Interrogative types in parental speech to language learning children: a linguistic universal? Research Report #54. December 1973.

4. D. Krus. Order analysis: A forward program for generalizable multidimensional analysis of binary data matrices. Occasional Paper #22. November 1973.

5. W. Bart. The pseudo-problem of IQ. Occasional Paper #21. October 1973.

6. J. Turnure & N. Thurlow. Verbal elaboration and the enhancement of language abilities in the mentally retarded: The role of interrogative sentence-forms. Occasional Paper #20. October 1973.

7. P. Dahl, S. Samuels & T. Archambault. A mastery based experimental program for teaching poor readers high speech word recognition skills. Research Report #55. September 1973.

8. R. Riegel, F. Danner & L. Donnelly. Development trends in the generation and utilization of associative relations for recall by EMR and non-retarded children: The BORTS test. Research Report #54.

9. R. Hoffmeister & D. Moores. The acquisition of specific referents in the linguistic system of a deaf child of deaf parents. Research Report #53. August 1973.

10. W. Bart & M. Smith. An interpretive framework of cognitive structures. Occasional Paper #19. June 1973.

11. C. Clark & J. Greco. MILDS (Minnesota Early Language Development Inventory) Glossary of Referents and Signs. Occasional Paper #18. June 1973.

12. J. Turnure. Interrelations of orienting responses, response latency and stimulus choice in children's learning. Research Report #52. May 1972.

13. S. Samuels & P. Dahl. Automaticity, reading and mental retardation. Occasional Paper #17. May 1973.

14. S. Samuels & P. Dahl. Relationships among IQ, learning ability, and reading achievement. Occasional Paper #16. May 1973.

15. N. Buium & J. Rynders. The early maternal linguistic environment of normal and Down's Syndrome (Mongoloid) language learning children. Research Report #51. May 1973.

16. T. Archambault & S. Samuels. A mastery based experimental program for teaching mentally retarded children word recognition and reading comprehension skills through use of hypothesis/test procedures. Research Report #50. May 1973.

17. W. Bart. The process of cognitive structure complexification. Research Report #49. April 1973.

18. B. Best. Classificatory development in deaf children: Research on language and cognitive development. Occasional Paper #15. April 1973.

19. R. Riegel, A. Taylor, & F. Danner. The effects of training in the use of a grouping strategy on the learning and memory capabilities of young EMR children. Research Report #48. April 1973.

20. J. Turnure & N. Thurlow. The latency of forward and backward generation responses in an elaboration task. Research Report #47. March 1973.

21. R. Riegel & A. Taylor. Strategies in the classroom: A summer remedial program for young handicapped children. Occasional Paper #14. March 1973.

22. D. Moores. Early childhood special education for the hearing impaired. Occasional Paper #13. February 1973.

23. R. Riegel & A. Taylor. A comparison of conceptual strategies for grouping and remembering employed by educable mentally retarded and non-retarded children. Research Report #46. February 1973.

24. J. Rynders. Two basic considerations in utilizing mothers as tutors of their very young retarded or potentially retarded children. Occasional Paper #12. January 1973.

25. B. Bruininks, J. Rynders & J. Gross. Social acceptance of mildly retarded pupils in resource rooms and regular classes. Research Report #45. January 1973.

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31. J. Turnure & N. Thurlow. The effects of structural variations in elaboration on learning by normal and EMR children. Research Report #41. September 1972.

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AMERICAN PSYCHOLOGICAL ASSOCIATION SYMPOSIUM PAPERS

(presented in Montreal, Canada September, 1973)

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University of Minnesota
Research, Development and Demonstration
Center in Education of Handicapped Children
Minneapolis, Minnesota

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Department of Health, Education, and Welfare

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INTRODUCTORY REMARKS

The Value of Relevant Research: Selling the Unwashed to the Pure

James E. Turnure

Symposium Organizer

In 1971, the Board of Scientific Affairs of the American Psychological Association stepped beyond its own professional arena and sponsored a symposium on pure and applied research at the annual meeting of the American Academy for the Advancement of Science, aimed at "increasing public understanding of the role of scientific psychology in solving problems faced by our society" (American Psychologist, 1972, 27, 932). At this meeting, Wendell Garner discussed several fundamental distinctions pertaining to the research process, defined as the acquisition of knowledge (see ibid., pp. 941-946), and also distinguished between the research process, pure or applied, and the application of knowledge. He convincingly demolished the myth that the scientist, or knowledge acquirer, accomplishes most when completely isolated from the problem solver, or knowledge applier. He then demonstrated how many of the important topics of today's "pure research" (selective attention, space perception, speech perception, etc.) were generated in applied, goal-oriented activities, primarily forced on psychologists by World War II. Thus, applied research has led to great improvement in the quality of pure research, and not the reverse.

In this monograph Turnure introduces the general theme of the symposium, which is, basically, the potential for rejuvenation of the whole Child

Development enterprise accruing through participation in relevant, goal-oriented research and development activities. He describes the mutally beneficial results to be obtained from well-trained and theory-minded researchers applying their knowledge and skills through direct involvement in "on-line" and "second-order" (Meehl, ibid., 932-940) problems of society (especially in education).

One participant (Wozniak) in the symposium develops the thesis that a parallel, salutary enrichment of theoretical and experimental activities through "real-life," goal-oriented involvement has never occurred in Child Development. Charlesworth demonstrates that meta-theoretical congruencies (e.g., ethological-ecological necessities) provide a broad framework into which child development research should adapt. Another participant (Moores) emphasizes the advantages of an R & D Center as one alternative, or supplementary, organizational arrangement to traditional departments, particularly as a means of promoting "relevant" research, and as a vehicle for a broader range of student training possibilities. Rynders serves as an exemplar of an "applied" researcher and Samuels as a "pure" researcher who relate their ongoing work to the previous concepts and arguments. Social acceptance and financial sponsorship, maintenance of conceptual and methodological rigor, and personal satisfaction are discussed as they relate to the R & D Center concept and to stresses on the profession.

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Relevancé and Rejuvenation

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In considering reasons why what we've termed small-p psychology in America should turn its interests and activities to the concerns of society, and as if these were not sufficient, to pointing out further causes for concern, I feel I have been goaded for some time by two related dissatisfactions, one being that little was being accomplished that seemed to make any difference in the affairs of the species, and, two, most of the ideas that seemed theoretically or intellectually interesting to me were being developed in foreign countries (with at least one exception being Chomskian linguistics). Not that there has been nothing to admire in American Psychology - our technical development in conducting research has seldom been approached anywhere. But to what end? Technique without purpose is like technology without values - but I'm not going to develop that analogy, I'd have too far to go to catch up to the mounting criticisms of undisciplined technological growth.

My remarks may be better developed by referring to another reason I preferred to speak in this slot, after Charlesworth and Wozniak, which pertains to the particular disciplinary positions espoused by the preceding speakers, both of which lend themselves in an exemplary way to one of the initial propositions to be advanced here. The ethological approach is reknowned

for its emphasis on close, painstaking, and long term observation of subjects under study, particularly as regards their constant adaptations to their environment. It has appeared to me likewise, that Russian research, and certain American derivations, such as the verbal self instruction work, can also be characterized as emphasizing continuous monitoring of subjects' ongoing adaptations to relatively lifelike tasks, almost to the same degree as the ethologist, although usually not for such prolonged time spans. In their close attention to the functional adaptation, moreover, these two approaches share what I consider to be the strong suit of what has been American Psychologies most effective contribution to applied efforts, thus far, the behavior modification approach. Of course, going beyond the observational level, great differences appear in procedures and conceptualizations, but my main point here is that the subject himself looms larger in these approaches than in other areas of American academic research and theorizing (admittedly, the subject may be something of an empty shell in the operant approach). I believe that this contention becomes convincing when one considers the extent to which individual cases are utilized descriptively and dispositively by each of these approaches. I feel I must also insert a reference to the early Piaget here, as well; and let's not forget Levin. By contrast it is my impression that most American research relies on one-time, or even multiple but still static observations, or suffers from what Shulman in the Review of Educational Research calls "meta-trialosophy", and so is bogged down in manifold ways of

managing our basic and very arbitrary, unit of analysis, the trial. I don't want to get myself bogged down in possibly beating a dead horse, although I don't think it's even lame but, my suspicion is that the characteristic American way with research methods (I refer to the "experiment" and the "test") as well as the conceptual approaches that go with them, are impeding rather than enhancing our understanding the nature of children's development. I will return to considerations of methodology later, but must push on with the present theme of how and why close observation of, participation in, and applied inquiry into the everyday activities of our subjects is necessary to produce theoretical accomplishments of a higher order than say, Paul Meehl's grandmothers'. In a 1967 paper entitled "Theory-Testing in Psychology and Physics: A methodological paradox" Meehl asserts that his late uneducated grandmother's commonsense psychological theories had, as he put it, non-zero verisimilitude. Now why can we be so sure that Meehl's grandmother was in possession of a number of satisfactory psychological insights, while it is so hard for any of our colleagues to convince us that they know hardly anything for certain about children or their development? Well, if you've read Meehl you know one reason has to do with the fundamental insufficiency of our present statistical designs to allow us to demonstrate anything conclusively; but another, and the one that is of most concern presently, is that, as I indicated above, few of us are studying children, while Meehl's grandmother probably was.

The call for all of us, and I would emphasize especially graduate students, to get involved in activities of what Meehl, in a more

recent article, calls "first-order relevance" is not original to me obviously, nor to Meehl. It is an idea whose time has come. Myrtle McGraw has recently pointed out the need for students of childhood to gather knowledge *in situ*, which can be used to guide our investigations *in extremis*, and so enhance the validity, and, perhaps, the usefulness of our experimental data. In concluding their recent article on "Cultural differences and inferences about psychological processes," Cole and Bruner discuss the nature of "relevant materials" and their assistance to the teacher in engendering psychological growth, and they say "It requires more than a casual acquaintance with one's students to know what those materials are": But earlier in that article Cole and Bruner had chided *psychological researchers* for having less than a casual acquaintance with the competencies of many of the children, and with a lack of resourcefulness in creating situations (conditions) in which these could be expressed. Finally, let me cite a not too distant precursor of the present proposition. Bill Kessen, in his illuminating book, *The Child*, introduces his theme of the historical rediscovery of persisting problems in child development and education by referring to the rejuvenating effect that the presence of their own children had on the studies of Darwin, Binet, Watson, Baldwin, Piaget, Preyer, and Tiedemann. Barring some modest proposal, say, such that AFDC children or some other population will be randomly assigned to graduate students entering the field, for their mutual enlightment, it appears quite obvious that future students, like most of us in the past, will be

deprived of the opportunity for early relevant education, unless we can begin providing field experiences that are effective. Ah there's the rub - because we must recognize that merely going out and observing, or interning, or, in many cases, even working in applied settings may be worse, in terms of biasing our conception of children, than absolutely no experience. An apt example for this audience may be the reinforcing nature of a visit by a student (or a professor!) from an S-R oriented psych department to the local neighborhood school just a few years ago, when both S-R le. .ng theory and its traditional classroom reigned supreme. In this case the methodological restrictions of the laboratory coincided with the administrative restrictions of the classroom in impeding the performances and masking the competencies of children available for observation. Now, I am not presently able to offer a definitive proposal for systematizing field experiences so that they will always provide rewarding contrast s (although we will presently hear a paper suggesting schemes for developing mutually rewarding Town-Gown interactions). However, I would hope that thoughtful consideration of my colleagues modes of working and their rationales for them would help develop some new operational concepts. For my own part, I have noted that all their work has a contemporary cognitive caste to it, and that they assume the skills investigated by each one embedded in complex and complicated cognitive, physical, and social systems. One outcome of this awareness, is a great deal of cooperation and collaboration among investigators in our Center. But what has impressed me even more, is that each of them has had the facility to maintain a cognitive

orientation while restraining an apparently omnipresent tendency bearing on cognitivists to relate their work to the pervasive and prevailing conceptualization of "cognitive functioning", which in its global mushiness is so abstruse as to be intractable to application. My colleagues, I find, have concentrated on more readily accessible, task-based aspects of real-field problems, and have brought their expertise to bear on what is to the handicapped child (and, I think to all children) a problem of "functional cognition", or how to develop and utilize specific cognitive capabilities of a broad variety.

Upon mentioning handicapped children let me note an historical connection between relevance and rejuvenation, and the somewhat peculiar appearing circumstances that all of the symposium presenters here are presently working not only on applied aspects of child development but applications specifically oriented to handicapped children. In the 1972 edition of his Historical Readings in Developmental Psychology, a review of the youthful wellsprings of our discipline, which included 37 aritcles, Wayne Dennis selected 13 or 36% which deal with exceptional children. This observation is inserted here to help convince you that working with the handicapped per se is not only relevant, but that such work can be rejuvenating in the sense of returning us to original, and in my opinion unsolved - but promising - problems.

At this point I intended to expound on the thesis that basic research can profit as much or more from applied research as the reverse, which had been the prevailing mythology for several decades

at least. However Wendell Garner convincingly set the record straight in the American Psychologist a little over a year ago, as had William Bevan before him in Science, and Leon Yarrow just recently applied the thesis specifically to work in child development in the latest SRCD Newsletter. But it does seem necessary to add something about revising training and employment practices in the field on the basis of this startling change in perspective. I simply cannot believe that the present haphazard system of selection for academic-cum-ecological niches, which actually begins prior to admission to college, provides a breadth of experience sufficient for independent thought or independence in career choice, and in problem area thereafter. These systematic problems, which I have only time to allude to, then interact with the "sociology of the profession", which includes status and reward systems, to produce enormous constraints on trainers and trainees which restrict choices, impoverishes experience and enlightenment, warps the aspirations of all, and distorts the distribution of effort so as to produce the crises of funding and anguish over employment that we are confronted with today. Recommendations have been made, such as moving to Federal loans for student funding, which in turn might make prospective students a little more thoughtful and independent in choosing training; or post-training work requirements for those accepting training funds, which might force departments to include training components which have some utility (utility is by the way what students generally mean when they refer to relevance, according to Menges and Trumpeter, 1972). I know this sounds

onerous to professionals and academicians who are devoted to free if responsible choice and actually feel they have it, but it is interesting that all of Garner's examples of beneficient applied work describe practical problems imposed on those whose subsequent basic research and theorizing benefitted from it. Of course there were circumstances which mitigated against the resentment of coercion which might have arisen in the conditions, and so precluded doing the good applied work which so effected subsequent increments in basic work. The solution to such a psychological impass arising contemporaneously may lie in assuming the attitude espoused by Harriet Rheingold, by "declaring a national emergency" on either the national level (like the "War on Poverty"), or at this, our professional level. The enemy she identifies is hatred, bigotry, selfishness, privilege and stupidity. (Alan Sroufe's earlier list included prejudice, materialism and authoritarianism incidentally.) She recommends commandering talent, as in the militaristic wars, so we could volunteer or be drafted, but in one way or another we must get involved. Indeed we may find that the present generation needs no such psychological ploys or prods.

I would like to close with two brief comments, one pertaining to perspective and one to standards of quality. Requiring involvement in the "field" application of work will give realistic comprehension of the limits of generality of laboratory research, and it is also conducive to engendering a realistic breadth of view regarding the actual complexities of "conditions" in which our "subjects" typically "perform", and in which our "independent variables" must effect their results. This latter prophylactic effect of field practice

against certain types of unnatural fixations attributable to specialization, much less "over-specialization", is one that has often been identified with teaching or perhaps I should say "good" teaching - for instance by Isidor Chein. My present level of awareness suggests to me that instilling some substance into the ideal of service, that at least some Universities nominally espouse, would contribute in return a great deal more of the type of experience I think is necessary for developing good judgment in formulating theories and implementing methodologies than does teaching. The next step in the complexification of our knowledge may have to come from immersion in the field just as after WWI & II.

Finally, I would just like to reiterate that there is no necessity to forego standards or abdicate commitment to intellectual excellence in doing applied work. The sources I have cited and the work of my colleagues and many others has convinced me of that. But I am convinced also that both theory and application would profit if everybody would go out among 'em in the field and mix it up a little.

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"Ethology's contribution to a framework

for relevant research"[1]

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Introduction

In a recent critique of various forms of philosophical analyses of the scientific enterprise, Stephen Toulmin (1972) points out that a perennial common concern of philosophers of science has been for the "acceptability" of scientific propositions. Acceptability, as Toulmin defines it, refers to the evaluation of propositions in terms of a set of a priori definitions which, by their very nature, are located within and controlled by the limits and rules of a particular school of thought. Philosophers occupied with acceptability have tended to ignore the problem of the "applicability" of scientific propositions, applicability referring to the evaluation of propositions in terms of standards, requirements, and demands of disciplines and human undertakings outside of the discipline in which the proposition itself has been developed. In evaluating this distinction, Toulmin suggests that philosophers of science shift their concerns from acceptability to concerns of applicability and in doing so open up philosophical analyais to the testimony of human problem solving experience, testimony that philsophers have historically tended to ignore.

[1]Efforts on this paper were supported by the Grant Foundation support to the Ethology Workshop 1973, by the Human Ethology Team, Max Planck Institute Percha/Starnberg, Germany as well as by the RD&D Center of the University of Minnesota.

Toulmin's suggestion has radical implications. It means that rational disciplines occupy themselves less with the time-honored problem of developing internal consistency and logical systematicity and more with matters of the function and adaptive value of those collective concepts and methods of thought which man has created to solve his everyday problems. Once scholars within a rational discipline such as philosophy decide to take Toulmin's suggestion seriously, they will, in his terms, be compelled to become "more historical, more empirical, and more pragmatic" (p. viii).

Toulmin's suggestion is also very appropriate for psychology today. It is the thesis of this paper that psychology, if it is to survive its present crisis will have to become more historical (in terms of evolutionary theory--to be explained below), more empirical (in a particular way -- also to be explained below), and definitely more pragmatic. This is especially true if psychology wants to be effective in helping man solve his problems. It is the second thesis of this paper that a powerful means towards helping psychology achieve this goal already exists in many basic concepts contained in the synthetic theory of evolution as it is currently being expressed in the behavioral scientists by the work of ethologists. Such concepts, in addition to having the capacity to connect psychology more firmly to the biological sciences, will also require that psychologists pay more attention to everyday problems of human behavior and adjustment. In this respect the research psychologist will, perforce, be brought into a stronger working relationship with practioners faced with problems confronting people in the outside world. In short, both pure and applied researchers, lab and field workers, academic

knowledge gatherers and everyday knowledge users will find a common working ground within the ethological framework that potentially can benefit both.

As a relatively young science with a wide range of opinions and internal disagreements about how ethological research should be done and what questions should be asked, ethology obviously cannot be expected to bring about a novel change in all the ways of psychologists. There are many substantive problems, such as language and most of the higher cognitive functions, which ethologists currently cannot face or simply refrain from facing (v. Blurton-Jones, 1972). However, during the short course of its existence, ethology has evolved an epistemically interesting and practically powerful set of concepts and methods which have so far yielded remarkable success in understanding animal behavior (v. for example Lorenz, 1937, 1970; Tinbergen, 1951, 1963; Eibl-Eibesfeldt, 1970).

As a branch of ethology, human ethology (as manifested, for example, in the pioneer work of Freedman, 1965, 1971; Eibl-Eibesfeldt, 1970, 1971; Blurton-Jones, 1972; McGrew, 1972) is even a younger science than non-human ethology, and as such has even more unresolved problems of content and method. But here, too, ethology's inroad into the domain of human behavior has already brought with it some new and interesting implications for research. As will be pointed out later, some of these implications are especially appropriate for issues concerning the relationship between pure and applied research and the relevance of such research for solving problems of everyday existence.

Ethology: the natural and artificial

As students of biology of behavior, ethologists are mainly concerned with how an animal goes about adapting to its natural environment. This concern is translated into at least six major questions: (1) what is the nature and frequency of behavior patterns the animal employs? (2) under what internal and external stimulus, including ecological, conditions does he employ them? (3) what individual and species function do such behavior patterns serve? (4) how do such patterns come to exist phylogenetically and/or ontogenetically? (5) what neurophysiological and endocrinological mechanisms underly such patterns? and (6) what status and distribution, if any, do such patterns have in other species? Ideally, empirical data on the nature, frequency, stimulus conditions, function, and comparative features of the behavior are gathered and related before attempts are made to understand how such patterns were acquired and what their underlying mechanisms are. In other words, the first phase of ideal ethological research is basically observational, inventory-descriptive, and correlational; the second phase more experimentally and psychometrically interventive and manipulative. Emphasis throughout both phases is upon the evolutionary significance of the behavior patterns--in short the significance, if any, of the behavior for individual and species survival within known ecological contexts. It is necessary to stress the latter since behavior, for the ethologist, cannot be understood without detailed reference to the environmental conditions under which a species as well as the individual develops.

What is important to emphasize in all this is that the starting and terminating point of ethology as an epistemic undertaking is the understanding of behavior in its natural context. This is true even if the ethologist is initially compelled to study his animal of choice, in semi-captivity, vivaria, or other highly artificial conditions such as laboratories. Unlike many comparative psychologists, ethologists generally do not find understanding behavior in lab conditions a satisfactory terminal point. To understand the animal in his natural habitat is the ultimate goal of an ethologist's research.

The question of what is natural and what is artificial has to be faced here since it is an important distinction underlying the main theses of this paper. Herbert Simon (1969) in his discussion of artificial intelligence has already successfully demonstrated that making such distinction is both possible as well as heuristically valuable despite the obvious conceptual, etymological, and semantic morass one can get into.

For purposes of the present argument all behavior and all environments in the broad sense of the term can be labeled as natural simply because they constitute part of the world as we know it. In this sense then it is possible to talk about man's artifact-making behavior and the artifacts produced by such behavior (his buildings, weapons, cosmetics, automobiles) as being as natural to him as a deer's running through a meadow and leaving his tracks in the soft ground. Both man and deer can be viewed in this vein as doing what comes naturally, the natural products of their behavior constituting a change in the environment which in turn could have natural

consequences upon their own behavior as well as that of others. Reducing the artificial to the natural by such an arbitrary definition is not unreasonable, however, it is unnecessarily impoverishing to ignore what distinctions still exist between the two. There are empirically vague differences in what is implied when we use both terms and some of these differences refer to identifiable properties which are necessary for the present discussion.

The identifiable properties that distinguish the natural from artificial may be assigned differentially to environments and behavior so let us entertain each separately. When we speak of an animal's natural environment we usually mean that the environment is typical for him and for his species, that it is an environment with major dimensions that have precedents in earlier environments within which the species developed, that it is an environment indispensable in most, but not all, respects for the survival of the animal and his species. We also usually mean by the term that it is an environment minimally influenced, if at all, by man and his artifacts, although in the last two to three thousand years this influence has grown mainly through the general effects of man's rapid intrusion into the animal world as well as through more specific effects resulting from domestication, breeding, zoo keeping, etc.

Artificial environments, in contrast, can be atypical for the animal, so atypical that they occur once and only once in the life of a single animal, or have absolutely no precedent in the history of the species or for the animal itself. A Skinner box and the particular shaping contingencies employed is a good example.

Artificial environments usually are dispensable--the animal can happily live without them. They can also be destructive and downright lethal, or they can be supportive and non-lethal. "Can be" is emphasized here since what an environment can be for an animal depends upon the animal's ability to respond to it as well as other contingency factors. This brings us to behavior.

In a crude sense all behavior can be viewed as the animal's natural way of responding to changes in his environment. Such behavior may be adaptive and highly successful or grossly maladaptive and ultimately unsuccessful; whatever the outcome, such behavior, in a sense, is all the animal has to work with. Here is where the problem of ability or the problem of the animal's capacity enters the picture. All animals within a species or across species obviously do not have the same abilities to meet environmental conditions. This is one of Darwin's main points. Environments can be so atypical that they far exceed the animal's ability to adapt and the animal perishes; on the other extreme environments can be so atypical the animal flourishes in unprecedented ways (and numbers). In either case, there is an asymmetry between behavior and environment which usually works in one direction only--the environment ultimately decides over the animal. As a rule the reverse does not happen, although we have to leave the possibility of there being one exception in the case of man (but I doubt it).

Man constitutes a big part of most animals' environment and in most instances the dominant part. Hence the asymmetry between certain subhuman animals and their environment is very great. Man's

idiosyncratic, frequently very atypical, unprecedented constructions pose tremendous survival challenges for animals, and for himself as well. Part of the back-to-nature argument is that man's ingeniousness in manufacturing the artificial has led to artifacts or productions which now stand in the way of his physical and mental health.

Scientists vary greatly in the manner and degree in which they intrude upon animals in their attempt to understand them. It is at this point where the initial decision of the scientist becomes very crucial. On one extreme of this the continuum are scientists who start research by observing the animal in his natural habitat with as little intrusion as possible (hereafter the naturalistic observer). The animal's behavior is directed toward and controlled by the stimulus conditions of its natural habitat and is relatively uninfluenced by the scientist. On the other extreme are scientists who start research by exposing the captive animal to stimuli varying in kind and intensity (hereafter the psychometrician) or by compelling the animal to undergo various non-specific conditions (early deprivation, enrichment, or trauma, for example) or to perform various prescribed responses (hereafter the experimentalist). In both cases the choice of stimuli, conditions, or responses may be highly atypical (or artificial) for the animal or may approximate (but not be equivalent to) the stimuli, conditions, or responses characteristic of the animal in its natural habitat. In either instance, the psychometrician and experimentalist both attempt to set up conditions in as simplified and ideal way as possible to maximize obtaining results which can be interpreted as unambiguously as possible. The naturalistic observer can never

achieve the degree of unambiguity reached by the former, however he has techniques for approximating it -- for example, he can observe the behavior of interest across a large number of varying and non-varying conditions (or subjects or species) and relate the various observations statistically or simply by means of common sense and analogy.

Obviously, a single scientist can be all three of the hypothetical scientists just mentioned at various periods of his research project and perhaps, in rare cases, all three concurrently. The ethologist traditionally has tended to start his approach to the problem by engaging in a relatively prolonged period of naturalistic observation and/or by mastering available naturalistic-observation literature. His aim is to know in as detailed a manner as possible how his animal behaves in his natural environment before formulating hypotheses about how and why he behaves that way.

In dealing with humans the ethologist's strategy seems as equally as reasonable. However, historically, psychologists have not taken such a strategy seriously. As a result, preciously little is known about how humans live in their natural habitats. With few exceptions such as the classic work, for example, of Barker and Wright (1951; 1955) and Barker (1963) most of which has gone under the rubric of ecological psychology (v. Willems, 1965 and Caldwell, 1968), the more recent combined psychometric and observational work of White and Watts (1973), the observations and insights of some clinicians, social workers, and one-shot empirical studies of animals and humans scattered here and there throughout the literature, most psychological research

has remained heavily on the end of the psychometrician-experimentalist point of the continuum, a point which Tinbergen forcefully criticizes in his introduction to Blurton-Jones (1972) book on the ethology of child behavior. Tinbergen's main point is that, unlike most subdisciplines within biology, psychology failed to develop a solid and comprehensive data base of naturalistic observations by rushing prematurely into the laboratory.

If it is really true that psychology has been prematurely artificial in its study of humans, then it is not difficult to understand why there is still such a big gap between research psychology today and problems of great social relevance. Psychologists are, as a whole, still quite removed in their research approach from understanding what it is that influences and conditions everyday human adaptation. Rather than emphasizing what humans <u>really do</u> as part of their reaction to the natural human condition, psychologists have tended to emphasize what humans <u>can do</u> in artificial psychometric or experimental laboratory conditions. It stands to reason that the latter run a much greater risk than that run by the naturalistic observer in turning up findings which fail to relate in any significant way to behavior outside of themselves. This is not to say that neurological tests of infants, measures of perceptual or intellectual skills, etc. are not relevant for understanding behavior outside the laboratory. However, it is a great mistake to assume from such test results that the skills measured by them are actually implicated in (or the only ones implicated in) interactions characterizing the individual as he meets in the world outside the lab. It also

does not mean that such skills have any real adaptive value for the person. Knowledge of everyday adaptive skills can only be obtained by first observing the individual in natural situations and then, if necessary, carefully developing measures or experimental situations that will help disentangle the complexity of the naturalistic data without distorting it.

The risk of highly artificial psychometric or lab conditions failing to tap everyday employed or survival important abilities is understandable in light of the nature of the subject psychologists have to work with. It is a well-established general rule that the more complex the animal's central nervous system the greater its behavioral variability and complexity of behavioral organization. It is a well-established fact also that all animals have behavioral mechanisms (as well as morphological, neurophysiological, etc.) that aid them to survive (v. Marler J. Hamilton, 1966; Hinde, 1966; Mayr, 1970; and Manning, 1971). Such mechanisms may be highly specialized in the sense that they work effectively within a narrow band of environmental variation. Or such mechanisms may be highly unspecialized and consequently effective in a very wide band of environmental variation. As Lorenz (1969), Rensch (1972), and others point out, man is the most unspecialized of all animals. This being the case, it is not difficult to understand how virtually any human could adapt to the psychometric or experimental demands posed him by the pscyhologist. By adapting what is meant here is responding to instructions or the situation, however, minimally in the expected manner, and thereby satisying the investigator. The thousand dollar question is whether the capacities underlying such adapations are related in

any significant way to those capacities with which the individual controls his everyday affairs. Predictive validity, ecological validity, face validity are some of the terms used to connect psychometric measures with what actually happens out in the real "natural" world.

This is not the place to assess the degree to which the various artificial approaches characterizing the psychological enterprise have failed in obtaining an adequate picture of natural, everyday human functioning and abilities. Making such an assessment would require that we already had a picture against which to compare the various approaches. We do not have this picture. However, there are enough instances where psychology's attempts to deal with socially relevant problems have been less than promising and it is most likely that this has been due to a lack of knowledge of what the human is like behaviorally in the real world. It is now general knowledge that the assessment of black children's language ability and general intelligence has had a bad start as well as many of the vast number of cross cultural studies which used methods and ways of thinking characteristics of a small percentage of the world's white population. In the same category belong the failure of early enrichment programs to produce significant long term (or even short term in some instances) effects on the level of children's cognitive functioning as well as the failure of numerous school programs to prepare children adequately for the major problems of adaptation in adult life (assuming that the schools have been sensitive to research in educational, developmental, and general psychology).

It might seem unfair to judge psychology so harshly; humans are more complex and unpredictable than any phenomenon under scientific study. Granting this, it still cannot be overlooked that psychology in the Western world has become a vast relatively well-funded enterprise with two major oft-stated goals--scientific or epistemic and technical or practical. Success in achieving the epistemic goal has so far been disproportionately greater than success in achieving the practical. Part of this disproportion is due to the simple fact that so much more is known about subjects in test and lab situations than in the outside world. But not only is there a dearth of reliable empirical data about natural behavior, there are many misconceptions about it. Chronic misconceptions about the behavior and abilities of persons of different race, social class, nationality, and culture than that of the researcher are a major example which are so blatantly present in the work of psychologists they need not be documented here. But lest psychologists get too depressed, it is worth noting that other sciences have trouble with misconceptions too.

A good example of having misconceptions because of not having the relative facts can be found in forestry. For years forest fires were condemned as bad because they destroy forests--or so it was thought. It took a long time until anyone in a high position took seriously Chapman, a young forester, who on the basis of careful observations pointed out that fires and healthy forests were not incompatible entities under certain ecological conditions and with certain species of trees (v. Wagner, 1971). In the meantime, Smokey Bear, a come-to-the-rescue product of a well-meaning psychologist,

came into being and soon brought large segments of the public under behavioral control. Fires were prevented at a rapid rate and immediately snuffed out when they were not prevented. But as a result, healthy forest reproduction was, much to the surprise of many, in some areas, significantly thwarted; underbrush normally burnt away by fires, accumulated at such a rate, that when a fire did start a holocaust resulted that destroyed older trees, trees that normally resisted "natural" fires. The lesson is clear. Psychologists should ask themselves how many similarly well-inten tioned producers of Smokey Bears are now trying to help the minds and behaviors of millions of children without knowing what these minds and behaviors are really like and how they function in the normal life of the child and also without knowing what environmental stimulus conditions are supportive of the growth of these minds and behaviors.

What much of the above boils down to is that we have at least three major research strategies available to us: (1) we can study exclusively what an organism can do--test for all his abilities, competencies by intervening into his life by various artificial means--as has been done in psychometric and experimental work, or (2) we can study exclusively what an organism actually does do without any kind of, or a minimum amount of, intervention on our part--as has been done in the few naturalistic observational studies of human behavior that now exist, or (3) we can study him both ways back and forth. If we did the latter would be able to generate a foundation of data that would both produce a comprehensive and coherent picture

of the phenomenon as well as have maximum applicability and impact on areas dealing with social problems. Most psychologists probably like to claim that, in one way or another, they do the latter. While this may be true, it has not been true enough throughout the history of psychology. This becomes evident if one considers the lack of success research psychology has had in both providing an integrated theory of human or animal behavior (v. Hodos & Campbell, 1969, for an example of the failure of comparative psychology, for example, to develop an integrated theory) and in producing novel and above-chance impacts on such human problem areas as education, mental health, and social behavior. If the wisdom of Paul Meehl's grandmother (recognized perhaps more in Minnesota psychology circles than elsewhere) allows predictions equal to or better than those provided for by research psychology, there is obviously no need to continue federally supporting the latter a few million times more lavishly than the former.

Ethology's contribution

There are three major ways ethology can contribute to psychology--conceptually, methodologically, and attitudinally. Ethology's conceptual contribution, as noted earlier, comes within the framework of the synthetic theory of evolution (Mayr, 1970) and thereby brings with it concepts familiar to biologists--concepts such as adaptation, natural and sexual selection, adaptive radiation, convergence, parallel evolution, behavioral and morphological analogies, and homologies (and variants thereof), phylogenesis, ecological factors, selection pressures and so on. These terms relate many domains of research interest and are both historical as well as contemporary

even when concentrated upon a single animal or species; they relate internal functioning and physical characteristics to external functioning and physical and social environmental factors as well as pull together phylogenetic and ontogenetic factors. In short, those terms are integrative in an extremely powerful and intellectually satisfying way.

Ethology's conceptual contribution can be more specific than just diffusely integrative, however. At least two such contributions can be mentioned here. One is in terms of species and historical comparisons, the other in terms of the concept of adaptation.

Viewing a human problem in terms of similar problems, if any, present in other species or in terms of putative problems facing Homo sapiens during his evolution can have a salutary effect in linking a particular, well-localized problem to environmental, species-historical, and possible phylogenetic sources. Understanding human aggression, for example, will not be achieved until all such links are empirically explored and conceptual schemes built to link the findings from such explorations. That there are attempts already being made in this direction (v. Johnson, 1972; and Hartup, w.w. & deWit, J., 1974) is very encouraging. Only an integrated picture based on cross-disciplinary efforts can be both intellectually satisfying and at the same time lay the foundation for effective social intervention into the problems raised by aggressive behavior.

The concept of adaptation is also a valuable guiding concept for both epistemic as well as pragmatic purposes. The term adaptation usually refers to the process whereby a species adjusts physiologically,

morphologically, and behaviorally to its environment. The term is sometimes used to refer to individual adjustment as well, in both an individually relevant sense (so the individual survives as long as possible) and/or in a species sense (so that the individual survives long enough to reproduce and care for the offspring, if necessary, until they can also reach reproductivity). Some writers, urge a distinction between the terms and suggest that adaptiveness be used for individuals and adaptation for supra-individual categories such as species. In dealing with practical problems of mental illness, education, and socialization individual adaptiveness is obviously the focus of attention. The point of emphasis in using such a concept to guide thinking is that each behavior, deviant or normal, can be viewed as adaptive relative to some specific or general internal or external condition. This concept is not new in clinical circles, but it still does not have a sound theoretical or empirical basis in such circles, and is not considered much by those in other circles such education and educational psychology (v. Charlesworth, 1973; and Charlesworth and Bart, 1974). Adaptive behavior can be viewed as restoring a pre-set level of equilibrium (v. Goodson, 1973). Such an equilibrium may serve a momentary survival need, a long-term individual one, or a longer-term phylogenetic one. Each of these should be considered as being implicated in any behavior; anything short of this would produce a fragmented picture and hense reduce the efficacy of any necessary practical intervention. Human sexual behavior, for example, is a good candidate for such an approach. Without understanding what ethologists mean by sign stimuli, IRM's,

fixed-action-patterns, bonding, sexual displays and the like, it is inconceivable that a satisfying and comprehensive picture of human sexual behavior can be achieved. Likewise, without understanding how sex roles are acquired, how modeling, and sexual identification take place, such a picture is also unattainable. It is not necessary to insist here that the picture we are talking about here consists of both learned and innate factors operating in sexual behavior; neither is the distinct contribution of each slighted nor the ways they interact during ontogenesis. The nature-nurture issue is not discarded, or revived in its old form by this approach--it is simply acknowledged for what it is--a heuristic way of viewing behavior which will lead to the inclusion of all of its determinative sources, whether they lie in early ontogeny in the form of early experience or somewhere in back generations in the form of genetically-controlled dispositions that have arisen through evolutionary processes.

The methodological contribution of ethology has already been indicated. Ethology rests heavily upon the now classical strategy of studying the spontaneous behavior of free moving animals by observing them in the natural habitat over relatively long periods of time. Today, this method has been expanded and differentiated in many ways and it would not be possible to describe the activities of many ethologists exclusively in these terms. However, the strategy still holds the key position in much current work with animals and humans by resting heavily upon the assumption that natural behavior, if observed long enough with care, and then accurately described, will reveal important information about its own organization, its function,

its relationship to other behavior, as well as suggest possible mechanisms that control it.

An immediate criticism of such approach is that it could quickly become an unwieldly, tedious, time-consuming cataloguing of uninteresting, familiar behavior that would be forever useless except for a few patient historians of 20th century behavior. While such a risk is great, it is possible to minimize it by taking seriously the implications of the conceptual and empirical scheme inherent in evolutionary theory. This scheme derives from the vast corpus of factual information on behavior collected across many species of animals and from the epistemic efforts made to classify and order such behavior into meaningful categories and functions. The ethologist observes within a preordained context provided by this scheme which by uniting information from comparative studies of behavior and morphology, anthropology, ecology, paleontology, genetics, and other related biological disciplines aids immeasurably in focusing efforts upon the predominant, high-frequency, survival-related needs and behaviors that characterize animals as well as humans. In other words, the ethologist does not look at his animal coldly and immaculately-free of preconception. He has his predispositions and biases about what is important. As a safeguard, however, against preconceptions generated by the scheme, the ethologist constructs the mechanics of his observational method carefully so as to achieve a balance between recording too much of the easily observable transient and redundant behavior and losing too much of the hard to observe permanent behavior which is critical for the animal's survival. The choice of behavioral units and their

application are problems labored over very heavily in the thinking and practice of ethologists (v. Blurton Jones, 1973; Smith, 1974). There is no space here to discuss further on how such problems are attacked. Rather it should be emphasized that the decision to focus on units is an important and complicated one. The decision has been worth it for ethologists, for it has turned out to be an empirically and logically sound strategy for dealing with observations of behavior both objectively and statistically, as well as relevantly for understanding the animal.

The strategy of emphasizing naturalistic observation as a first phase of scientific research is an old wise one. The results of such observation become the basis upon which future, more-controlled work is conducted. The source of ideas or hypotheses about a phenomenon are much better defined and much more valid if empirical studies into the nature of a representative sample of the phenomenon (in all its variations) are first undertaken. Intuitive notions about human behavior and its determinants are easy for everyone to come by. But, as one finds out sooner or later, such notions may grossly mis-estimate the importance and distribution of the phenomenon as well as misconceive its true nature, causes, and function.

The convenience gained by working on an intuitive, empirically uninvestigated notion about what is important to study about a particular phenomenon and how to go about it is very seductive. The same can be said for choosing one's subject. Using an animal that conveniently fits into a lab as a tool to derive general laws or to test deductions from a theory has been a familiar strategy in psychology. Justifying

such an approach is not easy, however, since it presupposes that there are such general laws that will operate when the context of the animal is totally different, for example, when the animal is out of the cage and away from artificial constraints. While such a strategy has proven to be of great merit in the physical sciences and most of the biological sciences, it may be of more doubtful merit in the behavioral sciences in its present state of affairs. The great complexity and variability of human behavior requires a long period of careful watching and recording to sort out what is worth studying and what is not. The rigorous experimental psychologist who chooses to work with a small piece of behavior without knowing its nature in the natural context and what the conditions are attending its occurrence and non-occurrence is similar to the man who lost his wallet in a dark alley, but chose to look for it on the main street because there was more light there.

It is much more reasonable to allow the salient problems of human adaptation to emerge from a study of these problems as they occur in the natural world and let the results of such a study serve as a policy guide to more controlled research than to depend upon a policy derived from a mixture of unexamined intuition, fact, and ideology. An ethologically oriented psychologist's commitment to an objective, naturalistic approach to practical problems will provide a solid basis upon which to construct intervention problems as well as a way for an unbiased assessment of them. As "experimental administrators" in Campbell's (1969) scheme, the ethologically oriented will justify the importance of the intervention on the objective reality of the problem and its importance

for individual adaptation, not on the certainty of the answer which is the goal of those working within the experimental paradigm.

There are many areas of human development and competency for which we have no reliable natural history data. Even in seemingly well-worked areas such as those dealing, for example, with mathematical abilities (the new math is a practical result of psychometric research on these abilities) there is still not enough knowledge of the natural development of number concepts. As Brainerd (1973) points out, comprehensive studies of the origins and natural development of number concepts are still lacking. As evidence that the new math, with its emphasis upon the individual number approach, was not built upon a comprehensive picture of the child's natural abilities, he offers evidence demonstrating that the order of emergence of various number concepts is ordination first, number second, and coordination third. Initial training on cardination, which is the main approach in the new math is, in his mind, out of place. While it is not in my province to test the validity of his allegation, his general point is well-taken. The natural history of intelligent behavior has not yet been written, Piaget notwithstanding (he began such a history with his infants, but became more psychometric the older his subjects the behavior of whom constitute the bulk of his monumental efforts). Some of us at Minnesota have started on a project in the natural history of intelligence and have found only a few related precedents for it, mostly in the animal literature such as in the work of Kohler.

The attitudinal contribution of ethology to the problem at hand is best summarized by two simple, non-scientific terms--"openness" and

"connectivity." The former refers to opening up a research paradigm to two external influences--interdisciplinary research and the world of human problems. The interdisciplinary influence has been considered as an ideal by numerous behavioral scientists for decades. However, such an ideal has not been fully realized. In an interesting monograph on ice age hunters in the Ukraine Klein (1973) points out, along with editors of the monograph series, the real need for interdisciplinary research and for publishing outlets which would acknowledge the important integrative value of such research. While Klein's work is in the area of anthropology it becomes immediately clear that the old boundaries of academic disciplines dissolve under the impact of the problem he works on--paleobotany, paleozoology, brain anatomy, ecology, and other related disciplines are all recruited to make sense of the phenomenon of ice age hunters. In human ethology a similar (but as of this date not as complete a project) has been conducted by Konner (1973) on infancy and childrearing in hunter-gatherers in Botswana. Konner tries to make sense of infant behavior in terms of birth variables, family planning, the reflexive capacity and feeding patterns of the neonate, early maternal behavior and other variables, such as ecological, characterizing the Bushman way of life. Jolly (1973), in a treatise on primate behavior, also shows great sensitivity to the interdisciplinary approach by discussing primate behavior in terms of ecological, social, physical, as well as human psychological (cognitive and behavioral) research.

Besides seriously opening itself up to interdisciplinary cooperation, the ethological approach to human behavior can also open itself to the world of human problems either indirectly through practitioners

or directly through research efforts. Fortunately, institutional mechanisms for both approaches already exist. Research, Development and Demonstration Centers, such as those at the Universities of Indiana, Oregon, and Minnesota are good examples of this approach where a network of both pure and applied activities are coordinated to attach particular adaptational problems faced by handicapped children. (v. Moores, 1973) In Germany the Max Planck Institute for Psychiatry in Munich is also a good example of institutional involvement, being based upon Emil Kraepelin's farsighted vision of a day when an interdisciplinary attack would be made upon problems of mental illness. Ploog (1972), present director of the clinical branch of the institute, is a strong proponent of Kraepelin's vision and argues persuasively why there can be no long-term productive separation between research efforts and therapy (between, for example, neurobiology and behavior modification).

While openness refers primarily to interdisciplinary behavior and institutions, "connectivity" refers to epistemic links between various disciplines and disciplines and real world problems which are generated by the openness. Such links consist fundamentally of propositions about particular phenomenon which converge from numerous methodological approaches upon a particular substantive matter. Webb et al. (1966) in their monograph on unobtrusive measures in social science research discuss the power and utility of bringing results from various different and independent measurement processes down to bear upon a particular substantive problem. In their terms "The most persuasive evidence comes through a triangulation of measurement

processes. If a proposition can survive the onslaught of a series of imperfect measures, with all their irrelevant error, confidence should be placed in it." (p. 3) In the present context triangulation would consist of a multitude of disciplines and outside sources bearing down upon a particular problem of human adaptation. As a result any fact or hypothesis generated by efforts within this framework of research would find itself imbedded in a broad matrix of information generated independently (as well as dependently) by practitioners and scientists in other disciplines. Such imbeddedness would mean that multiple checks would be made upon the statement's veracity and importance for the topic under study. A healthier atmosphere for a scientific statement cannot be imagined.

Conclusion

Too frequently scientists forget that their operations and artifices (their labs and journals, their technical jargon, federal and private grants) are means towards obtaining reliable knowledge rather than ends in themselves. Also too frequently do they come to believe that their conclusions somehow become the final standard of reality against which the outside world gradually must come to compare itself. Furthermore, some psychologists come to believe with an ingenuous arrogance that their artifices and the rationale underlying their use (most of which are built upon a narrow understanding of human behavior) can actually serve as programs upon which human behavior can and should be changed. Such forms of social intervention and blind reductionism are not part of science's traditional interest in knowledge per se nor in knowledge for the public good.

They are expressions of scientism which are politically or unconsciously motivated. They are also forms of epistemic activity which erroneously construct, by the means of ingenious artifices, phenomena to suit a narrow range of epistemic needs. They ignore nature to which a scientist, because he values objectivity above subjectivity and knowledge before action has committed himself to understand rather than to manipulate. If to manipulate his ideal is to do so only after he understands. Psychologists who have a difficult time understanding human nature may not, in frustration, try to manipulate or create it according to their own satisfaction and expect to get away with it. Intervening in the lives of people without knowing the natural conditions under which they live, what their problems of adaptation are, and what they expect to get out of life is no way of winning the trust of society.

Hence, it should be monotonously clear by now that to solve problems of health, education, and socialization reliable first-hand knowledge of the phenomena as they occur in natural environments is required. Without this knowledge it is unreasonable to expect better than chance success in dealing with applied problems. Ethology is not an applied science, but it is an absolute necessity for the ethologist to know the normal life behavior and natural habitat of the animal he studies. As a result of his continued involvement with the animal he may also get to know how the animal reacts to captivity and experimentation, but is is the former knowledge that serves as both a starting and main terminating point for all his efforts. Knowledge of behavior in natural habitats contributes first to structuring the questions to be answered by experimentation and secondly to interpreting the answers gained by

experimentation. Only on the basis of these two sources of knowledge can the ethologist successfully apply himself to problems of animal life.

At the present rate of human problem build-up an unplanned, single-disciplinary, laissez faire psychometric and laboratory approach to research is rapidly becoming unacceptable. As a result of increasing public pressure the laboratory psychologist encapsulated within his paradigm and totally concerned with the acceptability of his work (to others in his paradigm) rather than with its applicability outside the paradigm will become an anachronism. Society is demanding connections between the lab and the non-lab world.

Many psychologists like to include in their discussion sections of papers a statement to the effect that they have raised more questions than they have answered. For the scientific establishment such a statement is usually received as a sign of humility, maturity, and integrity. For the practitioner, who has to face everyday problems with sick, uneducated, handicapped and unhappy people, such a statement is received with no great joy for the simple reason that it means more delay and consequently more human suffering. According to historians, science is in the continuous process of opening up new areas for research while closing down others and turning them over to technology. Most areas of psychology are not developed sufficiently to have reached the latter part of the process. Psychologists need only ask themselves which areas of human behavior have been rendered intelligible enough to be turned over to practitioners and social technicians for meaningful application to real life problems. According to my limited view of the

field, behavior modifiers and some brain psychologists deserve the most credit for having made certain problem areas of maladjustment intelligible enough so that above chance, non-intuitive intervention can be possible.

As higher primates with great cerebral cortices, psychologists have a close to infinite capacity to behave in various ways. Hence they have an almost infinite capacity to produce more work for themselves, as long as the paradigm members on the whole approve. On the other hand, human subjects, who also have great cerebral cortices, have an almost infinite capacity to keep psychologists busy. One set of cortices releases responses from the other. satisfactions are achieved and the process continues. Sooner or leter constraints have to be put upon this mutually reinforceing relationship for the simple reason that this relationship has not borne results commensurate with its cost.

As I see it, three constraints can do the job mentioned above: (1) the constraint produced by the organization of the behavior itself as it occurs in the natural environment; this constraint guides the choice of variables and planning of experiments and replaces the constraints of convenience, fad, ideology and institutional rules governing reward, advancement, etc. (2) the constraint produced by accepting to work within the evolutionary framework; this is a paradoxical constraint since it requires engaging in interdisciplinary cooperation as well as accepting the heuristic value of evolutionary theory, both of which expand as well as limit the field of research operation and (3) the constraint produced by practical problems of human adaptation;

at first glance this may appear to be the most constraining since it suggests that applied problems instigate and control the movements of applied and pure researchers. This concern is not justified if one keeps in mind that the abnormal and normal, the socially deviant and accepted, the natural and artificial are all part of the same picture of being and becoming a human as well as of being and becoming a member of the animal world in evolution.

It is in these ways, then, that psychology can develop in the direction of becoming more historical, more empirical, and more pragmatic, as Toulmin urges for philosophical analysis. In his nomination ballot statement on issues facing psychology, Donald T. Campbell points out the necessity of validating scientific instrumentation against the experience of practitioners and against common sense even if the latter are not totally free from illusory and misleading elements. There is no need for pure researchers to feel threatened by such a challenge. The rigor of science is needed as an antidote to such elements just as everyday behavior in the real world is needed by the behavioral scientist. Neither excludes nor diminishes the other; with a conscious effort both can be mutually enriching. Ethology as a young science immersed in concerns for the animal in nature and in possession of a powerful theory as well as a sound scientific methodology can help serve as a way of solidly linking the complicated, confoumded world of social problems and the more simple world of scientific rigor.

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Moving Research Across the Relevancy Continuum

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The process by which the discovery of new knowledge is accomplished and eventually translated into educational innovation is a complex one which may be viewed as extending over a series of identifiable stages. Gallagher (Table 1) presents five phases: Research, Development, Demonstration, Dissemination and Adoption into an ongoing educational operation. Each phase requires a different emphasis, concentration of professional skills, and organizational support.

The ultimate criterion of successful educational research must be initiation of changes in the educational system which are of demonstrable benefit to children. Anything less than this should be unacceptable. A major component of any educational research must be careful consideration of the means by which results can be used to ameliorate the condition of children.

The present time lag in American education between the initiation of research activities and adoption of changes can be attributed to a number of factors. A basic obstacle is presented by the fact that the Research and the Adoption ends of the continuum have been perceived as the separate domains of universities and public schools respectively, two types of organizations which currently address themselves to different orders of priorities. At the university

level the priorities and reinforcements have been arranged in such a way as to encourage behavior which tends to concentrate on research activities to the exclusion of other stages. University based educational researchers of the highest prestige have been rewarded for conducting "basic" research exclusively. The outcome has been a closed system in which research is conducted frequently for the benefit of other researchers. In this way an individual might conceive of a problem, develop a design, run an experiment and then report the results in esoteric jargon, incomprehensible to the educational practitioner. Two inevitable outcomes of this system have been: 1) Much educational research has been conducted which is clearly irrelevant to education; 2) Much clearly relevant research which has been conducted has not been of educational benefit because of the lack of mechanisms for translating knowledge into behavior. Figure 1 illustrates the situation which exists when the interaction between universities and schools is nonexistent and where the translation of knowledge to action is blocked by misunderstanding and lack of cooperation between the two systems, resulting in an absence of activity in the Demonstration phase.

It is clear that the breakdown occurs at that point where university/public school cooperation should be at the maximum level; i.e., at the Demonstration stage which, in Gallagher's terms, involves an effective conjunction of organized knowledge and child. For any such conjunction to be believable it must be accomplished in a

school setting. Without an effective bridge, there is little confluence of knowledge and practice.

For the schools to progress, they must be open to inputs from a number of sources, with the universities providing a significant impetus for innovation. If the universities are to exert a major influence they must to a greater degree adopt a learner's role and be more sensitive to the needs of children and to the realities of the classroom. For an idea to be accepted it must stand the test of empirical verification in the field.

Ideally, both the schools and universities should function as partners in all phases of the Research to Adoption continuum. Although the universities should assume the major responsibility for the first stages, the schools must be able to influence the type of research activities undertaken. At the other end, the universities should contribute their unique skills to the evaluation and modification of programs which have been adopted into the ongoing educational operation. Figure 2 presents an ideal university-school symbiotic relationship.

In order to reduce the gap between research and practice, the University of Minnesota Research and Development Center in Education of Handicapped Children added a major Demonstration component to its mission so that new findings could be tested within an educational setting in order to ensure that innovations of demonstrable worth would be incorporated into educational systems more efficiently.

As individuals within the Minnesota Center have moved the

thrust of their research toward school based activities, it has become increasingly apparent that researchers face different problems than those encountered in more tightly controlled university based activities. Although the problems may not be any more difficult, they certainly at first glance appear to be. This may be explained in large part to the fact that most of us have been trained to design, conduct and report our research within a tightly controlled highly unstrained framework? The reward systems of the universities serve to keep a majority of investigators within this framework.

The move into the classroom represents both a relative loss of control and the introduction of numbers of potentially confounding variables. Members of the Center are in the process of acquiring the skills necessary to meet the demands of conducting relevant applied pedagogical research.

It should be stressed that we are not talking merely about the social and beaurocratic skills which involve dealing with variegeated grouped of children, teachers, principals and assorted administrators. Although this type of expertise is essential I am referring to issues such as sampling techniques, assessment of change, instrumentation, formative and summative evaluation, and development of behaviorally defined objectives. As growing numbers of scientists bring their talents to bear on these areas I believe we will witness the development of new relationships between psychologists and educators which will prove of greater benefit to children than our present system of non-overlapping closed sets.

Table 1

Phases of Translation of Knowledge to Action through Organizational Support

Developmental Phase	Purpose	Supporting Organizations
Research	The discovery of new knowledge about handicapped children or about those intellectual and personality processes that can be applied in these children	These are usually research centers and institutions, often found in universities, which can provide organizational support for long range attacks on difficult research problems.
Development	Knowledge, to be educationally useful, must be organized or packaged into sequences of activities or curricula that fit the needs of particular groups of children.	Sometimes done through research and development centers which concentrate on sequencing of existing knowledge; basic setting is still the university.
Demonstration	There must be an effective conjunction of organized knowledge and child. This conjunction must be demonstrated in a school setting to be believable.	A combination of university or government and school cooperation required. Usually, the elementary or secondary school is the physical setting and additional resources are supplied by the other agency.
Implementation	Local school systems with local needs usually wish to try out, on a pilot basis, the effective demonstrations they have observed elsewhere to establish its viability in a local setting.	Additional funds for retraining personnel and for establishing a new program locally are needed. Some type of university, state or federal support is often needed as the catalyst to bring about this additional stage.
Adoption	To establish the new program as part of the educational operation. Without acceptance of the new program at the policy level, demonstration and implementation operations can atrophy.	Organized attempts need to be made to involve policy decision makers (i.e., school board members, superintendents, etc.) in the developmental stages so far. Items like cost effectiveness need to be developed to help make decisions.

From Gallagher, J. J. Organization and special education. Exceptional Children, 1968, 34, 485-491.

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Figure 1

Traditional Perceived Disjunction of the Missions of Universities and Schools

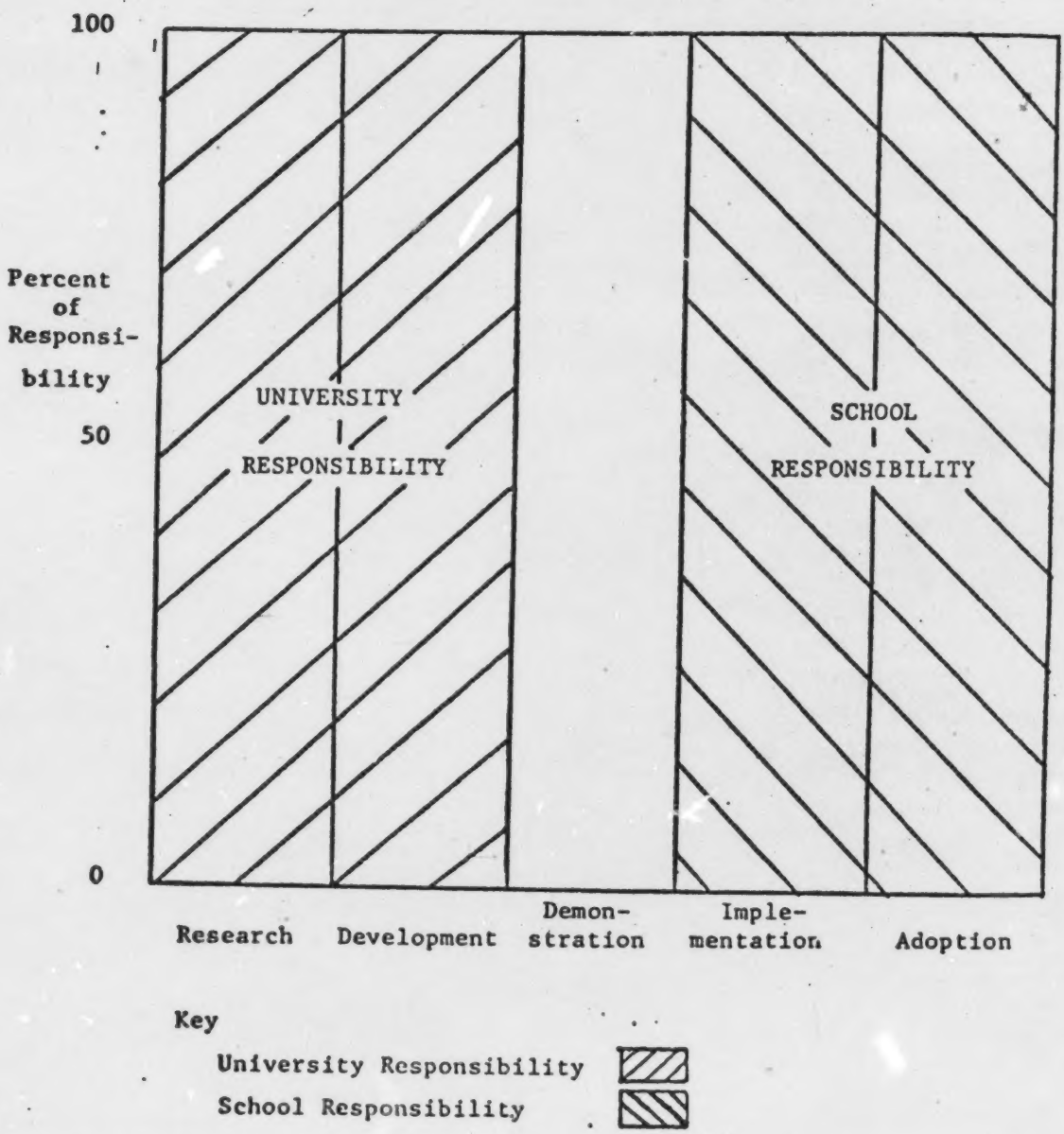

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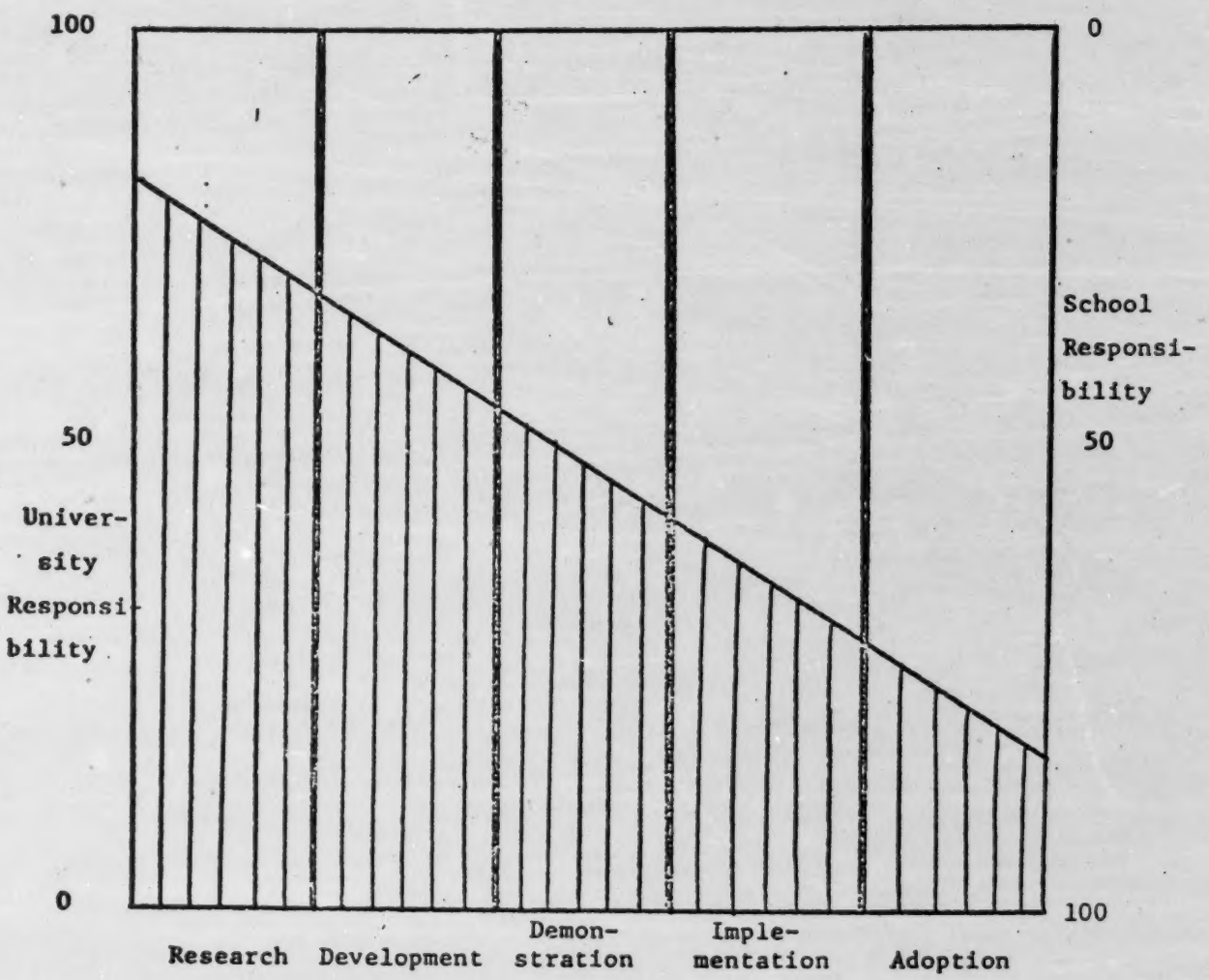
Figure 2

Ideal University/School Sharing of Responsibility



Project E.D.G.E.: A Case Study of Research and Development Relevancy

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Our discussion this afternoon has two purposes, (1) to provide an overview of Project E.D.G.E., an early intervention project for retarded children, and (2) to describe how an early intervention study benefits by being associated with an RD&D Center, and, in turn, strengthens the Center.

Project E.D.G.E. (Show slides of the program)

Project E.D.G.E. is a federally funded, longitudinal early intervention project. Its goal is to maximize communication abilities in Down's Syndrome (Mongoloid) children through language tutoring beginning in infancy and lasting until each child is five years of age. Since its inception in 1969, the project has been lodged within the University of Minnesota's Research, Development and Demonstration Center in Education of Handicapped Children which has received continuing support from the United States office of Education, Bureau for Education of the Handicapped (BEH).

As we begin outlining the study, one might ask, "Why conduct an early intervention study with these children? Is the problem of

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[1]E.D.G.E. stands for Expanding Developmental Growth through Education. The project is funded under Grant #OE-09-332189-4533-032.

sufficient magnitude to warrant a longitudinal investment?" Our answer to the question is a resounding "yes." One out of every six or seven hundred children born in the United States suffers from a chromosomal anomaly known as Down's Syndrome (mongolism). About seven thousand children with the condition are born in this country each year (Kramm, 1967). Part of the rationale for early intervention rests on the fact that the IQ scores of Down's children tend to decrease as a function of increasing chronological age (Dameron, 1963; Carr, 1970). At maturity, most have IQ scores which fall in the severe (IQ 25-39) or moderate range (IQ 40-54) of intelligence (Robinson & Robinson, 1965). This pattern of diminishing IQ score with increasing chronological age, coupled with the fact that Down's children reared in institutions tend generally to have lower IQ scores than those reared at home (cf, Stedman and Eichorn, 1964; Shipe and Shotwell, 1965), has greatly increased parents' desire to raise their Down's children at home, at least during the early years of life. This desire, translated into powerful lobbying efforts, has done much to stimulate the development of community services for educating and caring for Down's children. But despite the creation of these community services and some evidence that an educational stimulation program can enhance the Down's child's development (Matkin & Molloy, 1970; Rhodes, Gooch, Siegelman, Behrns and Metzger, 1970) virtually no formal educational services are available to these children prior to the time they are eligible for a day activity center program (usually not before the age of three). Project E.D.G.E. was designed to fill the Down's Child's first five years of life with daily sessions

of affectionate, focused, one-to-one instruction. From three months of age until two-and-one-half years of age he will be tutored one hour each day, usually by his mother, through a set of interesting activities which have been developed for him. Objects and toys, e.g., blocks, a doll, bubble soap and a straw, fingerpaint and 16 other items, were chosen because of their own particular qualities (some leave a trace, some have supposed social value, some have to be put together in a serial order, etc.) Employing a set of instructional techniques (and being careful not to dampen their own individualistic style), mother's initial tutoring sessions emphasize the labels and characteristics of objects, and, then a bit later, place emphasis on the spatial-temporal and logico-mathematical relationship of objects and of several objects. Piaget and others have contended that these relationships are the "raw material" of intellectual development.

From the time the children are two-and-one-half years of age until they are five-years old they are enrolled in an experimental preschool program where the emphasis continues to be on communication development.

Preliminary analyses of data indicate that project children are significantly ahead of a group of home reared Down's children who serve as controls, on measures of receptive language, muscular development, task orienting behavior, and IQ. These data must be viewed with great caution, however, since the differences may be evanescent in this relatively early stage of the project. Nevertheless, these results are encouraging and suggest that early intervention

may be important, perhaps crucial, for maximal communication development in Down's children.

Advantages of Conducting an Early Intervention Study in an RD&D Center.

The early intervention literature is rife with examples of studies which have been less than adequately conceived, executed, and evaluated. The RD&D Center in Education of Handicapped Children, at the University of Minnesota -- acting as a research mechanism -- helped Project E.D.G.E. improve these three aspects of intervention.

Improved Conceptualization. Before our project was admitted to the RD&D Center, several entrance criteria which are integral to project conceptualization had to be met. For example, the major investigators had to present a convincing case that the intervention would culminate in important educational products for handicapped individuals assuming that the intervention was successful. Thus, time-lines, clear delineation of the questions of interest and an event calendar were required. Documentation was reviewed by both an internal RD&D Center committee, by field readers for BEH and by the BEH Advisory Committee itself. Such review resulted in several changes in design and methodology for Project E.D.G.E. at its inception.

Improved Quality Control. Longitudinal studies, particularly early intervention studies because of their relative newness on the RD&D scene, can profit from a good deal of constructive criticism lest they go off the track or, perhaps more seriously, go off on

too many tracks simultaneously. In our own case we are required to defend our design and methodology at least once each year before a general meeting of the RD&D Center membership. Then, at the end of each calendar year, we must justify continued funding in writing. The first stage of this justification takes the form of a written progress report, reviewed by an internal RD&D Center Research Committee for continuation purposes. The committee may reject the project for continued funding, recommend substantive or conceptual changes or approve it without reservation. If the progress report justifies the project's existence, major investigators must next cast it into a formal proposal, subject to final Center Director review, and then submit it for approval by field readers, the BEH Advisory Committee and BEH officials. Several changes in Project E.D.G.E.'s conceptualization and design have resulted from these reviews over the years.

Improved Educational Products. A focus on the development of worthwhile educational products for handicapped children is the hallmark of projects in R&D Centers funded through BEH. Project E.D.G.E. is no exception.

During 1972, special funds were set aside by BEH for product development in the RD&D Center. These funds were not tied to any project. Because of them, we were able to have some of our project's activity scripts developed by artists as prototypes for eventual publication. These funds also permitted us to document several aspects of our project in motion picture form for eventual editing and dissemination.

Another example of product development in Project E.D.G.E. which was brought about by the RD&D Center was the development of a consortium of RD&D members who worked with us to develop and modify instruments to measure the syntax, grammar and articulation characteristics of Down's children's language. This mutually beneficial collaboration, developing through a series of Center meetings, led to a battery of integrated language measures which could not have emerged without the Center acting as a mechanism for its members.

We have attempted to provide evidence that an early intervention project can be strengthened through affiliation with an R&D Center and can, in turn, strengthen the Center as a whole.

In concluding we strongly support the concept of researchers working <u>together</u> to improve individual as well as collective research and development enterprises especially those involving complex field components such as early intervention projects. Collective effort cannot help but benefit the researcher, the research group, and the research community at large.

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How Psychologists Can Be Relevant Or You Can Have Your Cake & Eat It Too

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> "To the great Greek scientist Archimedes, the study of mathematics and physics meant far more than pure scholarship. Imaginative application of laws he worked out led to eminently practical inventions--from contrivances employing the level to an ingenious steam-powered cannon."*

There is a belief among psychologists that one either has to be a pure or an applied scientist. There is also a belief that the pure scientist does his work in the refined atmosphere of the laboratory, cut off from the practical problems of society while the applied scientist does his work in the unrefined atmosphere of the clinic, school, hospital, or the street, cut off from the research problems and concerns of the experimental psychologists. It is the purpose of this paper to emphasize that many theoretical findings of the "pure" psychologist had their origin in practical applied problems. As a matter of fact, it can be argued that in order to solve many of the important practical problems in society, theoretical research must be done. In this sense, the applied scientist and the pure scientist can be one in the same; that is, you can have your cake and eat it too. Like Candide, many applied psychologists are the happiest of people in that their applied concerns carry them into theoretical realms.

Unfortunately, today one finds a growing conflict among the members of the faculty in many university departments across the country This conflict is often expressed as a false dichotomy between the "simple-minded" vs. the "muddle heads." The simple-minded are those

*Time Magazine, November 26, 1973, p. 58.

who have difficulty discovering anything interesting or relevant about the mind, but their methods are good and their findings are reliable. The muddle heads are those who investigate interesting problems and who make claims to having discovered a lot of interesting things about people, which unfortunately are not true. Again, it should be stressed that one need not be either simple minded or muddle headed about the approach one takes to work.

Not only are psychologists facing up to the apparent dilemma regarding the problems of doing work in either applied or theoretical fields, but one can find examples of the same type of dilemma in other academic departments as well. For example, Geri Joseph reported in the Sunday, November 11, 1973, Minneapolis Tribune that the English Department at the University of Minnesota is confronted with a similar type of problem. Joseph reported, "One professor explained the conflict this way. There are 2 groups within the English Department. One has chosen the academic world as a place to study, read, do research and have a flexible work schedule. Professors in that group know they must teach too, but they are more interested in their own scholarship. They think the department should do what it has always done--teach literature of the past and some of the present. On the other side of the fence, the professors see themselves as teachers and activists. They are less concerned with research and publishing articles. They want to move the English department in new directions--minority literature, for example, and film making. They believe they should be deeply involved with students and spend time with them after class in debate and discussion."

Funding for research reflects the shift in priorities from pure research to a more balanced program of both the pure and the applied. From 1968 to 1970, grant money for basic research increased 21%. But in the same period of time, money for applied research increased 90%. Influenced perhaps by these priority shifts, we are finding many more well trained experimental psychologists who are going into applied fields and using their expertise to solve some of the more critical problems our society is facing today.

Before proceeding, it might be wise to consider what is meant by the term "relevant." Generally, research is thought to be relevant when it can be applied to solve some problems. However, in evaluating and classifying research we have to take into account the degree of importance of the problem which is being investigated. Similarly we must look at long term and short term goals. In considering the relevancy of a problem we must also ask "relevant for what" and "relevant for whom." Paul Meehl has used the terms "first order relevancy" and "second order relevancy." First order relevancy is a term that might be used when one is working directly on the problem, such as the clinician working with the patient, the teacher working with the student or the behavior modifier working in a hospital setting. Second order relevancy is a term which may be used when one is doing research on how to improve the effectiveness of the clinician, the teacher, and the behavior modifier. Thus, according to Meehl, both types of work are relevant, but one class of research would be directly on the firing line whereas the other type of research would be passing the ammunition to those who are on

that firing line.

One of the problems we face in trying to determine the relevancy of a research project is that some types of research may not be immediately relevant to a problem but others may discover uses for the information. For example, impressed with the Jakobson and Halle work on distinctive features in phoneme production, Eleanor Gibson used the distinctive feature concept and applied it to the area of visual learning. Then Samuels and Williams used the Gibson distinctive feature work and applied this concept to problems of teaching children how to recognize letters of the alphabet when reading. In this instance we can note how information which was perhaps developed not for any particular applied problem was used later in the solution of a problem facing children in the acquisition of reading behaviors.

The dichotomy between those who do theoretical research and those who do applied research was reflected in the early United States Office of Education concept of how to bring about changes in education. The Office of Education funded the establishment of two laboratories in our country. One type was designated as a research and development laboratory where it was hoped that theoretical work would go on that would have bearing upon some of the more formidable problems encountered in education. The regional labs were established in order to do applied research. It was thought that the regional labs would utilize information developed by research and development centers. As one looks at these two types of centers today, it is easy to notice that the original concepts have been modified substantially. For example, at the

University of Wisconsin R&D Center, Wayne Otto is working on applied problems, namely how to measure the attainment of reading skills. At Teacher's College (Columbia) Joanna Williams is working on the problem of helping children who have learning disabilities. In both cases, although the work is applied, Otto and Williams find that a substantial amount of pure research must be done in order to solve the applied problems.

There are a number of problems in doing applied research which deserve recognition. The Federal government has funded research projects which were specifically designed to overcome current problems facing our society. Some of these projects have suffered from lack of continued support because of changing governmental priorities. In the mid 1960's U.S.O.E. funded the cooperative research programs in first grade reading instruction. The purpose of this study was to determine if any particular reading method was especially effective or ineffective for pupils of high or low readiness. A second large scale effort in reading also supported by U.S.O.E. was Project Literacy. The purpose of Project Literacy was to achieve a greater understanding of basic processes of reading through a interdisciplinary effort. Governmental support for research in the area of reading was continued by United States Commissioner of Education Allen. He was of the opinion that every child had a right to read and it was under his auspices that the Targeted Research in the Reading Program was funded. This was to be a long term, large scale effort to do the necessary research required to improve reading instruction in order to allow all children to be literate. The project was funded and the first phase of the project was completed, culminating in a

final report entitled "The literature of research in reading emphasis on models." The new Commissioner of Education Sydney Marland reviewed the target Research in Reading project and decided he would discontinue funding it. We can see here an example of research which was begun that had to stop because of a lack of support.

There are a number of problems that one encounters with applied research which are of a political nature. Politicians, who are deeply affected by the practical problems of society, often want quick results. Since many of the problems which we face in the area of education are long standing, it would seem that they would tend to be resistant to fast solutions. Politicians are also very sensitive to the amounts of money spent on research as well as to the outcomes of this research. During the last decade, the federal governmnet has supported intervention programs. Numerous intervention programs have not been able to display any advantage to the groups getting the special treatment. Consequently, the current political climate is such that continued funding for intervention programs is in doubt. Still another problem which faces the reseacher who is doing applied work is that there are often short funding periods, which frequently leads to the omission of theoretical research.

As mentioned earlier, there have been several criticisms of applied research. One of the criticisms has been that the research which has been done does not attack the problem in any meaningful way even though the problem that is being attacked is an important one. Still another criticism has been that the research did not

accomplish its stated goal. For example, one of the criticisms which has been directed at Head Start has been that students in Head Start programs did not have any substantial increase in IQ. One may question, however, whether or not the purpose of Head Start should be to raise a child's IQ. When we have poorly conceptualized goals, such as the purpose of Head Start being to raise the child's IQ and when the methodology which is available to achieve these goals is weak, it is little wonder that we find little or no advantage to those groups which are in intervention programs.

Still another problem which one faces in doing large scale applied studies is that it is extraordinarily difficult to locate suitable control subjects. In Project Followthrough, which was done on a massive scale throughout the United States, project after project failed to find any difference between experimental and control groups. One of the reasons for this failure to find differences was that the control group for one project often turned out to be the experimental group for some other project.

In summary then, the changing priorities of the federal government which lead to nonsupport, the desire of politicians to get quick results, the short funding periods which lead to the omission of important theoretical studies, the poorly conceptualized goals and weak methodologies available in applied problems, and the difficulty in getting suitable groups, point out the many problems one encounters in doing applied research.

As mentioned earlier, many of the important areas in theoretical psychology had their origins in applied problems. The work on selective attention had its origin in the 1950's. Aircraft

controllers hear numerous simultaneous messages from pilots. The problem was could the controllers keep the competing calls properly sorted out. This problem led to the work in the U.S.A. and in Britian on selective attention and memory using the methodology by Broadbent on dichotic listening.

The work on vigilance also had its origin during World War II when radar operators were found to be inattentive to the stimuli which were appearing on their scopes.

Another problem which had its origins in World War II was how do pilots gauge distance and height in landing a plane. It was hoped that information on this question would lead to a reduction of plane crashes. Dr. J. J. Gibson formulated his ground theory from direct observation of pilot behavior and experiments in the field.

In the area of individual differences, a practical concern of the French government was how does one determine which children would profit from schooling. This problem led to the development of paper and pencil tests to determine which children would do well in school.

Robert Gagne, who was asked to develop a system for training pilots, concluded that the theories and hypotheses which were used in experimental psychology were not useful in solving the practical problems of instruction. Consequently, he developed the procedure for task analysis, which has been useful in a variety of applied situations.

One can go on citing instances of theoretical research which had its origin in applied problems. However, the examples cited above provide just a sample of the link between the theoretical research which is going on today and practical problems in society awaiting solution.

In conclustion then, there are several routes for psychologists to take who wish to be relevant. For first order relevance the psychologist identifies an important problem facing society. The problem should have face validity in the sense that the man on the street or the client would recognize the problem as being important and worthy of an investment of time and money. For second order relevance the psychologist does theoretical research on a problem of concern to the applied psychologist. It is necessary for the psychologist who does work on second order relevance to keep ever in mind the practical problems which he is addressing. Hopefully, the sharp distinction between the applied scientist and pure research scientist will break down as more psychologists work in both areas in order to overcome the pressing practical problems facing society today.

In-context Research on Children's Learning as a Basic Science Prophylactic: or True Purity Doesn't Need to Wash[1]

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> "The child in school also has been studied, often in the context of projects on curricula or the use of mechanical and electronic aids in teaching. Such material has been omitted [from this book] on the grounds that it appears to have greater implications for educational practice than it does for the understanding of learning (Stevenson, 1972, p. xiii)."

The separation of "educational practice" from the "understanding of learning" implied in the above statement (taken from the preface to Stevenson's comprehensive review of experimental research in children's learning) accurately captures, in my opinion, a remarkable characteristic of the historical development of experimental child psychology. This is the almost complete alienation of the study of learning in children from the context in which a major share of that learning takes place, namely, the school.[2] Although a careful historical analysis of the genesis of this division would be impossible to present in the time available, there is, nevertheless, one major aspect of the development of the field of children's learning to which I would like to call attention as both a product and a

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[1] Paper presented at the 81st Annual Meeting of the American Psychological Association, August 1973, in a Symposium entitled: "The Value of Relevant Research: Selling the Unwashed to the Pure."

[2] The major, and perhaps the sole, exception in this regard has been the relationship which has existed between operant psychology and the classroom in terms of both programmed instruction and behavior modification. Although some of the arguments in this paper have implications for a critique of operant psychology, this will not be pursued here; and, therefore, further reference to research in children's learning should be taken specifically to exclude research originating within an operant framework.

continuing cause of the separation of the learning laboratory from the school. This is the fact that almost without exception, the major sub-areas in the experimental psychology of children's learning have been generated not by the observation of children engaged in the process of learning but by the appropriation by child psychologists of experimental paradigms and attendant conceptual baggage from both the animal and adult learning literature.

Thus, for example, the study of children's discrimination shift behavior as a function of age (Kuenne, 1946; Kendler and Kendler, 1959) derived immediately from the work of Spence (1936, 1937) and his students on transpositions and reveral-non-reversal shifts in rats. Hypotheses for the first studies of verbal pre-training in acquired equivalence of cues (Birge, 1941) and of pre-experimental deprivation on the facilitative effects of social reinforcement (Gewirtz and Baer, 1958) were drawn directly from Hullian (1943) behavior theory. Initial, investigations of learning set in children (Shepard, 1957; Koch and Meyer, 1959) were triggered by Harlow's (1951) suggestion of a relationship between an organism's phylogenetic level and rate of learning set acquisition. A small portion of Broadbent's (1958) work with adults on selective attention fairly swiftly found its way into the child learning literature (Maccoby and Konrad, 1966); and, of course, early work with children on conditioning (Krasnogorski, 1909), paired-associate learning (Norcross and Spiker, 1958), the delay (Lipsitt and Castenada, 1958) and scheduling (Kass, 1962) of reinforcement, non-reinforcement (Penney, 1960), and the effectiveness of secondary reinforcement (Leiman, Myers, and Myers, 1961) simply extended paradigms

and concepts previously employed with animals or adults to children.

One effect of this historical fact appears to have been the development of a strong inclination on the part of many experimental researchers in children's learning to regard not the learning of the child per se but rather prior research and theory about the learning of the child as the major or only source of their experimental questions. Experimental psychologists interested in children's learning have, in fact, traditionally looked more to the journals than to the classroom (or even to the direct observation of children engaged in learning in the laboratory) for their inspiration. As a consequence, the results of their investigations seem often to have had more to say to their colleagues about the characteristics of their methods and their theories than to the educational practitioner about the characteristics of children's learning.

It is interesting to speculate concerning how this situation, once it developed, could have remained as well tolerated as it has been by both the basic researcher in children's learning and the educational practitioner. Perhaps one major factor in this continued tolerance has been a mutual and uncritical acceptance of what might possibly be termed the "stockpile" myth. This is the view, borrowed from the phsical sciences, that there can exist a duality between the process of conducting pure and basic science in order to yield a stock of general, independently verified, relatively solid and unchanging facts, and the process of technological application in order to develop programs and techniques based upon whatever subset of this class of facts is found at any point in time to have become helpful. From this perspective, the pure scientist is freed from any

responsibility to see to it that the facts which he discovers are facts worth discovering since by definition any fact, by virtue of the fact that it is a fact, is a fact worth discovering. The pure scientist is accountable only to intersubjective testability and replicability and the justification of his existence is that he is participating in a process of building up a knowledge base which someone, somewhere, may someday be able to use.

In the area of children's learning, at least, it is clear that the conduct of science from the "stockpile" perspective has largely failed to enhance practice by providing facts which to date anyone has anywhere been able to put to use; and it is at best questionable whether the facts so far produced will even anytime in the future be of practical value. Such a science has clearly not been good practice; but must it be? The answer most frequently given to this question is, of course, that good science does not have to be good practice; only good science. The fact that little in the way of information helpful to the practitioner has so far been generated by experimental research in children's learning has been simply a function of the inadequate state of our knowledge. It is just that we need more facts; and if the science is allowed to remain basic and pure, it will sooner or later evolve to a point at which useful facts will begin to be generated. This view, in my estimation, overlooks a critical issue, one which bears directly on the validity of the "stockpile" notion. This is the question of whether science, or at least social science, conducted from a "stockpile" perspective can now even be considered to be good science, let alone good practice.

Although there has always been some variation in terminology, the basic, general definition of the task of science has remained reasonably stable for many years. Science is, essentially, a transpersonal search for knowlege and understanding, where to "know" is to discover relationships between observable events (the descriptive enterprise) and to "understand" is to fit such relationships into more comprehensive, organized,relational systems (the task of explanation). What we know, however, as psychologists and particularly as developmental psychologists exposed to the thinking of Piaget (1952, 1970, 1971) and of Soviet psychologists such as Vygotsky (1962, Leont'ev and Luria, 1968) Leont'ev (1959, 1972), and Rubinstein (1959, 1973; Payne, 1968), is that our view of what it means to know and to understand and of how that knowledge and understanding are achieved on a personal level has been altered greatly within the last few years.

From the structural-dialectical point of view shared by these thinkers, the world is still seen, as for the naive realist, as an objectively existent, ultimately knowable reality, waiting to be known. It is the nature of knowledge itself and the process whereby that knowledge is achieved which has come to be viewed differently. For Piaget and for Soviet psychology, the world exists in a state of constant change and development, and the epistemological task of the child or, for that matter, the adult is to bring order and stability to this change by seeking the organization and structure latent in the objects and phenomena of reality. Since this knowledge, this organization and structure is <u>latent</u> in reality, since it is an unobservable which underlies the phenomenal real, the epistemological process is

essentially constructive. It is a process of constructing structure. What is most important for our purposes here, however, is that from the very beginning the development of such structure is viewed as resulting from a dialectical interaction or interpenetration of the internal characteristics of the individual with the external reality of the environment; and that this interpenetration is actualized, refined, and corrected by human activity.

Thus, for example, for Soviet psychology, human knowledge is seen as a "reflection" of external reality "refracted" by the inner conditions of the knowing subject. Reflection and refraction are two sides of the same process. On the one hand, the form and content of thought is determined by the external world; while, on the other hand, the effect of the external world is determined by the inner characteristics of the form and content of thought. This leads to a progressive historical approximation in both the individual and in society as a whole toward absolute knowledge and understanding, toward an accurate reflection of reality, refracted through the inner characteristics of the knower; and this in turn raises the question of the criterion by which the truth of a reflection is to be judged. For Soviet psychology, this criterion is "practice"--human activity in the midst of the practical problems posed by the conditions of everyday life. Thus they are fond of quoting Lenin (1929) quoting Engels to the effect that "the success of our actions proves the correspondence of our perception with the objective nature of the object perceived (p. 109)."

The implication of this view is that knowledge and human activity

in a real, practical, every-day context are mutually interdependent. Knowledge of the world acts to guide and direct the activity by which man alters his context; and activity in context acts to correct and develop man's knowledge.

For Piaget, at this level of analysis at least, the process of cognitive development is seen in much the same fashion, as determined by the interpenetration of transforming activity or "assimilation" guided by the internal characteristics of the individual or "assimilatory schemes" with a reality-oriented adjustment of this activity under the influence of the environment, or, in other words, "accomodation."

If, as I feel we must, we apply this conception of the nature of the development of personal knowledge to the development of science as a transpersonal search for knowledge, the major implications are obvious. What science knows cannot be divorced from what science does; and, in particular, from what science does in a concrete, practical reality. Not only is good practice dependent for its direction on valid knowledge structures which constitute the legitimate product of science; but the process of developing good and valid knowledge structures and hence good science is dependent for its correction and refinement on practical in-context activity. In the Piagetian vocabulary, science must accomodate to the exigencies of practice if it is to avoid the development (as seems to have occurred in children's learning) of assimilatory schemes so heavily weighted to the side of assimilation as to preclude the possibility of ever achieving the functional equilibrium prerequisite to major cognitive developmental growth.

Before concluding, and hopefully to avoid potential misinterpreta-

tion due to my stressing the adaptational rather than the organizational aspect of the growth of scientific knowledge , I would like to briefly note that this is by no means to argue for either an abolition of laboratory research or a mindless return to purely naturalistic observation for its own sake. The issue which I am raising is not so much an issue of method in itself, although it certainly has methodological implications, as it is an issue of questions and answers--an issue involved with where the child learning researcher (or for that matter, any research psychologist) goes to seek the questions which he poses and attempts to answer (whether he seeks his answer in the lab, in the school, or in the home) and where he takes the answers at which he arrives for verification.

Hugo Munsterberg had, it seems to me, one side of the truth in 1908 when he wrote that:

"if experimental psychology is to enter its period of practical service, it cannot be a question of simply using the ready-made results for ends which were not in view during the experiments. What is needed is to adjust research to the practical problems themselves and thus, for instance, when education is in question, to start psychological experiments directly from educational problems (p. 8)." To this, however, must be added a second side -- that it is equally true that if the experimental psychology of children's learning is to enter a period of true scientific productivity, it cannot be a question of continuing to ignore the child learning in-context, in the classroom or the home, as a fruitful source of experimental questions and of continuing to fail to reality test the results of

our investigations by using them to guide our in-context action. On the contrary, if we adopt such an in-context approach, it may, as the title of this paper impeies, serve the function of a "basic science prophylactic" helping us to prevent any future occurrences of misconceptions of the type under which the psychology of children's learning has long been laboring.

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